ROYAL ASTRONOMICAL SOCIETY.

Vol. XVI. *April* 11, 1856. No. 6.

Manuel J. Johnson, Esq., President, in the Chair.

Rev. W. Jackson, M.A., St. John's, Workington; and
Geo. Roberts Smalley, Esq., Head Mathematical Master in King's College School,

were balloted for and duly elected Fellows of the Society.

On the Values of the Diameters of the Planets having measurable Disks, as determined with a Double-Image Micrometer attached to the East Equatoreal of the Royal Observatory, Greenwich. By the Rev. R. Main, M.A.

"The observations, of which the results are here discussed, extend over a long space of time,—namely, from the time of the invention of the double-image micrometer by Mr. Airy in 1840 till the year 1851. At the commencement of the use of this instrument, it was considered that one of the most important applications of it would be for the determination of the diameters of planets, its range extending considerably beyond that required for the measurement of the system of *Saturn*, and its accuracy within that range being far superior to that attainable with a wire-micrometer. In former papers I have discussed those observations made by myself for determining the ellipticity and the form of *Saturn*, as well as those for determining the magnitude and the breadth of his bright rings; and I have thought it of importance at the present time to collect all the measures of the other planets made by myself, and to give definitively the results for all, as obtained from a series of observations made by the same person with the same instrument and in a uniform way.

"The instrument with which the observations were made was not identically the same during the whole period of time. The chief changes which occurred were in the years 1849 and 1851, when advantage was taken of some arbitrary conditions in the ratios of the focal lengths and distances of the lenses, to ensure greater flatness of the field and greater distinctness of the images near the borders. In this latter construction the power is altered by the change of the eye-glass nearest the object-glass of the telescope.

"Previously to the year 1849, the value of a revolution of the

micrometer-screw, as determined by observations admitting of no doubt, was 17″·00. For the power used with the new eye-piece in 1849, commencing with February, the value of the revolution was assumed to be 14″·59, and, finally, for that constructed according to M. Valz's arrangement of the lenses in 1851, the value of a revolution for the power actually employed was 7″·23."

After a few additional remarks, the author proceeds to give the details of the measures which formed the groundwork of his researches. The remaining part of his paper is devoted to a discussion of those measures and the deducing of the definitive results.

Mercury.—This planet is so rarely visible in the evening, and so difficult to be measured in full daylight, that the author's materials for determining its diameter were not very abundant, although they are probably sufficient for obtaining a moderately accurate result. The definitive value of the diameter corresponding to the unit of distance is found to be 6″·89.

Venus.—In the discussion of the measures of this planet, the author takes into account the probable effects of irradiation. Considering, first, the daylight observations, he separates the measures into two classes—those made near inferior conjunction, and those near superior conjunction; and then forms two systems of equations of condition involving the constant of irradiation and the diameter of the planet as the unknown quantities. By a simple combination of the equations of condition in each set, he obtains two final equations, by the solution of which he finds the apparent true diameter of the planet corresponding to the unit of distance to be 17″·61, and the constant of irradiation to be −0″·61. A rigorous treatment of the equations by the method of least squares gives him 17″·55, and −0″·50 for the values of the same quantities. It would appear from the resulting value of the constant of irradiation, that the measured diameters are in reality too small by 0″·5, and that the contacts have been made too closely by the quantity 0″·25. This amounts, in fact, to saying, that on account of the feebleness of the light at the cusps and borders of the planet, the true cusp or border is not observed, but a point lying within it by 0″·25, a result which might have been in some degree expected. A discussion of the evening measures of the planet shows them to be totally free from the effects of irradiation,—a startling and unexpected result for a planet apparently surrounded with so large a mass of coloured diffused light.

The author next proceeds to discuss the measures for the breadth of the illuminated disk. The results for the daylight observations exhibit a most satisfactory agreement with that deduced for the measured diameters, while the evening observations show that the portion of the disk measured is also less than the true illuminated breadth, though by a smaller quantity. "Hence," says the author, "on the whole we may affirm that for evening measures of *Venus*, made with the double-image micrometer and telescope here employed, no correction is needed for the

effects of irradiation at the limb, and that at the boundary of light and darkness the faintness of the light causes the contacts to be made at a distance 0″·36 from the point where the sun's light really touches the planet."

Mars.—The author finds the diameter of this planet at the mean distance to be 9″·84. He has also obtained $\frac{1}{62}$ for the ellipticity of the planet—a result which he considers to be a tolerable approximate to the true value.

Jupiter.—The series of observations made for the determination of the size and form of this planet is very extensive, and a synopsis of the results is given in a table, which shows the degree of agreement of the separate determinations. In this table the corrections due to the want of illumination of one or the other equatoreal portion of the disk are given, but not applied to the equatoreal diameters; but they are taken into account in the calculation of the ellipticity and of the equatoreal diameters at the standard distance. This distance has been assumed so as to be identical with M. Struve's value, in his paper on the measures of *Jupiter* and *Saturn* (*Mem. R. A. S.* vol. iii.) namely, 5·20279. The ellipticities given in the table express, as usual, the values of —

$$\frac{\text{Equatoreal diameter} - \text{Polar diameter}}{\text{Equatoreal diameter}}$$

and their general agreement is as satisfactory as can be desired, and prove abundantly that the ellipticity $\frac{1}{13\cdot8}$ assigned by M. Struve is too large. As an additional proof of the accuracy of his definitive value of the ellipticity, namely, 0·05934, or $\frac{1}{16\cdot84}$, the author has calculated by means of it and the observed value of the equatoreal diameter, on each day when oblique diameters were also observed, the values of the oblique diameters, and has compared them with the observed values. The calculation is made in the following way.

Let a and b be the equatoreal and polar semi-axes of any ellipse, and r the radius-vector (referred to the centre) inclined at an angle θ to the major axis:—

Then

$$r^2 = \frac{a^2 b^2}{a^2 \sin^2\theta + b^2 \cos^2\theta}$$

Let ε be the ellipticity

$$= \frac{a-b}{a} = 1 - \frac{b}{a}$$

therefore $b = (1-\varepsilon)\,a$

and r^2 (after reduction)

$$= \frac{a^2(1-\varepsilon)^2}{1-(2\varepsilon-\varepsilon^2)\cos^2\theta}$$

whence

$$r = a(1-\varepsilon)\left\{1 + \left(\varepsilon - \frac{\varepsilon^2}{2}\right)\cos^2\theta + \frac{3}{2}\varepsilon^2\cos^4\theta\right\}$$

Now if, as is the case for very nearly all the observations of oblique diameters, $\theta = 45^{\circ}$, then,

$$r = a(1 - \varepsilon)\left(1 + \frac{1}{2}\varepsilon + \frac{1}{8}\varepsilon^2\right)$$

or,

$$2r = 2a\left(1 - \frac{\varepsilon}{2} - \frac{3}{8}\varepsilon^2\right)$$

or oblique diameters at angle 45°

$$= \text{equatoreal diameter} \times \left(1 - \frac{\varepsilon}{2} - \frac{3}{8}\varepsilon^2\right)$$

If, then, we assume $\varepsilon = \frac{1}{16{\cdot}84}$, we have,—

$$\text{Oblique Diameter} = \text{Equatoreal Diameter} \times (1 - 0{\cdot}03102)$$

and by this formula the computed oblique diameters have been derived from the observed equatoreal diameters. These have been compared immediately with the means of the two transverse diameters corrected for phase, and the results are contained in the following table : —

Date of Observation.	Mean of Observed Transverse Diameters.	Computed Transverse Diameter.	Excess of Computed Diameter.
	"	"	"
1840, May 16	42·65	43·25	+0·60
June 10	41·70	41·30	−0·40
1841, Feb. 19	34·88	34·51	−0·37
1842, July 15	46·57	46·86	+0·29
1849, Feb. 17	42·80	43·14	+0·34
Feb. 26	42·33	42·37	+0·04
Feb. 27	42·25	42·26	+0·01
Mar. 20	40·85	40·24	−0·61
Mar. 21	40·41	40·27	−0·14
Apr. 7	39·00	38·95	−0·05

The mean of the excesses of the computed above the observed diameters is, therefore, −0″·03, which shows, in the first place, that the ellipticity has been rightly assumed, and, in the second, that the planet is strictly of a spheroidal form. Hence the author assumes definitively,—

$$\text{Equatoreal diameter at distance } 5{\cdot}20279 = 37{\cdot}91''$$
$$\text{— — at distance unity} = 197{\cdot}24''$$
$$\text{Ellipticity} = \frac{1}{16{\cdot}84}$$

After a few remarks on the general smallness of the measures of 1849, the author concludes with a synopsis of the several definitive results contained in his paper.

Extract of a Letter from the Rev. W. R. Dawes to the Astronomer Royal.

"I have the pleasure of inclosing an account of my observation of the lunar occultation of *Antares* on the 26th, which was as satisfactory as the wretched condition of the atmosphere would permit. From many other observers, as well as from myself, thanks are due to you for so pointedly calling attention to this phenomenon. I confess I was not previously aware of the peculiarities you mention as having been formerly noticed in the occultations of this star.

"*Wateringbury*, 1856, *March* 31."

On an Occultation of Antares by the Moon, 1856, *March* 26. By the Rev. W. R. Dawes.

"At $15^h\ 20^m$ G.M.T. I directed my equatoreal refractor of 8 in. aperture to *Antares*, which the bright edge of the moon was approaching. The state of the air was very bad, the star being often puffed out to an enormous size, and exhibiting various brilliant colours. The moon's edge was boiling. After trying several different magnifiers, an equiconvex lens producing a power of 254 was preferred, because with it the small companion was occasionally seen separated from the large star, when, for a few moments, the latter drew itself up into smaller compass. The air would not bear a higher power; and with a lower the small star was constantly enveloped in the blaze of the large one.

"The *time* of the disappearance was not very accurately noted, my attention being directed almost entirely to the appearance of the star. Nothing decidedly remarkable was noticed previous to the disappearance of *Antares*, which took place instantaneously, and precisely at the moon's edge. There was a small depression in the edge at the point of disappearance. The small companion was not discerned at all for about one minute previously. The moon was quite free from cloud.

"The reappearance at the dark edge of the moon was well observed. The equatoreal was not used between the disappearance and reappearance; and being well carried by its driving clock, the phenomenon occurred in the centre of the field, and was seen at the first instant. The small companion appeared instantaneously at its full brightness; but from its diffused condition, it was not possible to form any correct judgment of its magnitude, which, however, could scarcely exceed the 7th. *Its bluish green colour was very conspicuous.* When I had counted about 18 beats of my pocket-chronometer, equal to $7^s{\cdot}2$, *Antares* itself suddenly blazed forth, and completely enveloped its companion in its enormously diffused image. Vision was even worse than at the disappearance; and I could not afterwards obtain a glimpse of the companion, though the strong twilight favoured its visibility.

"I fear there is some uncertainty in the observed interval: for my attention was so absorbed with examining the colour of the

small star, that the counting was carried on mechanically; and the startling effect of the burst of light at the close increased the tendency to error. But I hope the uncertainty does not exceed one or two beats of the chronometer.

"From the decidedly blue-green tint of the small companion, while *Antares* was still occulted, it seems evident that the colour really belongs to the star, and is not merely the effect of contrast with the deep red of the large star. As far as *artificial* occultation can decide the point, this seems to be the case with many other double stars whose components show a somewhat similar contrast of colours. In the present instance it was a peculiarly favourable circumstance that the *re*appearance happened at the moon's *dark* limb, as the possibility of the judgment of the eye being influenced by the light and colour of the moon itself is thereby precluded. To secure this, I employed a small field of view in the sliding diaphragm of my solar eye-piece.

"From the interest attaching to this object, and the extreme difficulty of procuring reliable measures of it as a double star in these latitudes, I am induced to anticipate the publication of the results I have been able to obtain of some observations of its position and distance. Having accidentally heard, in the spring of 1847, that *Antares* had been discovered to be close double by Professor Mitchell of Cincinnati, but being unacquainted with any particulars respecting the relative proportion of the components or their angle of position, I determined to try, on the first favourable night, what could be made of it with my 6⅓-in. Munich refractor. The following are the results which I obtained in that and the two subsequent years:—

	Pos.	Obs.	Wt.	Dist.	Obs.	Wt.	Power	Mags.	Epoch
	°			″					
a)	273·98	2	4	...	...	...	435	1½.8	1847·297
b)	273·63	9	67	3·471	10	42	435	1½.7½	,, ·299
c)	270·08	5	19	3·642	4	14	252	1½.8	1848·557
d)	271·62	5	20	3·418	4	16	252	1½.7	,, ·591
e)	275·97	5	20	3·247	4	12	375	1½.8	1849·406

Notes.

a) A, *red*; B, *blue*. No doubt about the companion. Became foggy. Observation worth little.

b) A, *red*; B, *purple?* Surprisingly well seen: a most beautiful and delicate object.

c) A, *red*; B, *purplish*. Daylight.

d) A, *red*; B, *very blue*. Daylight. Beautiful object.

e) A, *red*; B, *green*. Occasionally very well seen, but very unsteady.

Mean Result.

P=273°·17; obs. 26; wt. 130; on 5 nights
D=3″·457; ,, 22; ,, 84; ,, 4 ,,
} Mean Epoch, 1848·02

"If the angle of position were nearly coincident with the meridian, it would be almost impossible to observe the small star

in these latitudes, as the bright star forms a strong prismatic spectrum in that direction. This atmospherical effect may, however, be in a great measure counteracted by using a single lens as an eye-glass, or by the ordinary double micrometer eye-piece. The star being placed towards the southern (upper) side of the field of view, the eye-piece spectrum may be made very nearly to neutralise the atmospheric spectrum, and a very tolerable image obtained under favourable circumstances. The star was always thus treated while under measurement; otherwise no reliable measures of position could have been procured.

"*Wateringbury, Maidstone.*"

Note on the Occultation of Antares, observed 1856, *March* 26, *at the Royal Observatory, Greenwich, by Mr. H. Breen.*

The instrument used was the telescope of the altazimuth, a 5-feet telescope of 4 inches aperture. Nothing remarkable was noticed at the disappearance; the star disappeared instantaneously, although it was connected with the bright limb for several seconds. At the reappearance, the star was at first seen as a small point; and several seconds (probably 10 seconds) elapsed before it showed itself in its full lustre.

Note on the Occultation of Antares, March 26th, 1856.
By S. C. Whitbread, Esq. F.R.S.

"Soon after three o'clock, mean time, on this morning, I had a very good view of the occultation of *Antares* by the moon. The disappearance behind the bright limb of the moon was instantaneous, as if the star had been swallowed up. On the reappearance taking place, I saw a small speck emerge from behind the dark limb, which I should describe like a star of the twelfth magnitude, and in seven seconds *Antares* burst forth in full splendour.

	h m s
The disappearance took place at ...	15 43 33
Reappearance — —	17 2 16
	° ′ ″
The latitude of my observatory is ..	52 6 0 N.
Longitude — —	$0^h\ 1^m\ 39^s$ W.

"*Observatory, Cardington, near Bedford,*
"*March* 27, 1856."

Measures of the Exterior Diameter of Saturn's Ring.

By Professor Kaiser.

(*Extract of a Letter to the Editor.*)

"At the end of last year I received the double-image micrometer which I had ordered of Mr. Simms, but different circumstances prevented me for a few months from making use of it. A favourable opportunity for employing it first offered itself in the month of March, when I resolved to measure the planet *Saturn* with it. On the 10th of March the sky was tolerably transparent, but cloudy weather then ensued, and it was not until the 25th of the same month that at length there occurred a succession of several clear days. During this time, however, there blew incessantly a strong wind from the north or north-east, accompanied with intense cold; and the air, which was very transparent, was affected with such violent undulations, that *Saturn* appeared like a confused mass of light, so that to execute any exact measures was altogether impossible. Under such circumstances, the complete measurement of *Saturn* with the double-image micrometer being impracticable, I was obliged to wait for more favourable weather, in order to examine M. Otto Struve's theory by my own measures. I thought, however, that in the meantime I might be enabled to contribute something to the examination of the hypothesis communicated by Professor Secchi in the *Monthly Notices*, vol. xvi. No. 3. For this I only wanted to measure the exterior major axis of the outer ring, which is by far the easiest to measure; and I thought that a change in a few hours of nearly a whole second, if it existed, could not fail to reveal itself also to me, notwithstanding the unfavourable condition of the atmosphere.*

"If the air was tranquil for a moment, the micrometer exhibited pretty distinct images; the instrument, however, cannot be used for minute measurements without great circumspection. A principal difficulty appears to me to consist in the circumstance that the result of the measurement depends on the point of the field of view in which the images coincide. The variability hence arising increases with the distance to be measured, and may for a distance not exceeding two minutes produce an error of a considerable part of a second. With the highest magnifying powers, the measurement can therefore hardly be extended to two minutes, but the influence of the variability upon the following measures of *Saturn* is certainly very small.

"Supposing, that the value of a revolution of the screw amounts

* The author here alludes to an inconvenience which he has experienced in endeavouring to determine by the aid of a wire-micrometer the value of a revolution of the screw for a high magnifying power. Mr. Airy in the description of his double-image micrometer (Introduction to *Greenwich Observations*, 1840), has shown how the value of a revolution of the screw may be obtained independently of the use of a wire-micrometer.—Editor.

to 6″·430, I obtained for the major axis of the ellipse formed by the outer edge of the exterior ring of *Saturn* the following results :—

1856.	M. T. Leyden.	Major Axis Observed.	Major Axis Reduced to Mean Dist.	Deviation from the Mean.
	h m	″	″	″
March 10	9 23	42·26	39·46	+0·04
	9 48	42·12	39·33	+0·17
25	8 50	40·87	39·23	+0·27
	9 35	41·05	39·40	+0·10
	10 12	41·06	39·41	+0·09
26	7 15	41·21	39·62	−0·12
	9 17	41·30	39·71	−0·21
	10 6	41·20	39·61	−0·11
27	8 32	41·02	39·51	−0·01
	9 41	41·14	39·63	−0·13
	10 12	41·21	39·69	−0·19
28	7 40	40·96	39·52	−0·02
	8 57	40·65	39·22	+0·28
	10 0	40·93	39·49	+0·01
29	7 20	40·80	39·44	+0·06
	8 40	41·07	39·70	−0·20
30	7 20	40·99	39·69	−0·19
	8 40	40·86	39·56	−0·06
31	7 20	40·73	39·50	0·00
	8 35	40·77	39·54	−0·04
			Mean, 39·501	

"The second measure of the 10th of March was executed by Dr. Oudemans; all the others are by myself.

"All the above measures have been executed under the most unfavourable atmospheric conditions, and it is not to be doubted that the double-image micrometer admits and will furnish in future much more exact measures. It is remarkable that all the above measures accord incomparably much better with each other than those which Professor Secchi has communicated in the *Monthly Notices*, vol. xvi. p. 52. After the application of a correction, that diminishes the differences between his results not less than 0″·732, the latter continue to exhibit deviations from the mean, which are not less than those of my measures without the application of any corrections. My measures, therefore, do not seem to indicate any trace of a fluctuation by which the apparent major axis of the ring increases or diminishes to the extent of 0″·732 in an interval of $7^h·214$. A fluctuation in a period of $7^h·214$ may remain concealed for weeks if the ring is measured daily at the same hour; for if the ring is measured the first day just in its mean value, this will be nearly the case for many days together at the same hour, and the fluctuation may exist without

disclosing itself even to accurate measures. My measures, however, have been executed at sufficiently different hours to make such a concealment of the fluctuations almost impossible, and it is not probable that the errors of my measures have followed with precisely the same magnitude the same period in an opposite direction as the change which Secchi thought he had discovered in the major axis of the ring of *Saturn*. I hope to execute under a better condition of the atmosphere more complete and more accurate measures of *Saturn*, but the result of this preliminary inquiry appeared to me sufficiently interesting to communicate it briefly to you.

"*Leyden, April* 3, 1856."

Occultations Observed at the R.N. College, Portsmouth, by Capt. Shadwell, R.N.

1856				M.T. Portsmouth. h m s
Feb. 16	45 Geminorum	Mag. 6	Immersion. Dark Limb	11 52 36
Mar. 11	33 Tauri	Mag. 6	Immersion. Dark Limb	9 27 54
,, 13	136 ,,	Mag. 4½	Immersion. Dark Limb	8 48 31
,, 26	α Scorpii	Mag. 1½	Immersion. Bright Limb	15 20 50·5
			Emersion. Dark Limb	16 39 50·5

All good observations. In the case of *α Scorpii*, the star at the immersion appeared to hang on the moon's limb for about two seconds; the time recorded is that of final disappearance. The reappearance was instantaneous.

Note on a presumed Occultation of a Star by Saturn.
By Frederick Brodie, Esq.

"On 30th January observed *Saturn*, and was surprised to find that there were apparently four satellites plainly visible interior to *Titan*. I could glimpse a fifth occasionally. After a period of 2½ hours, I perceived that the one nearest the planet, then about 30″ distant from the edge of the ball, had not moved, but rather approached the planet; while the satellite next to it, *Tethys*, had altered its position with respect to it nearly 90°. I suspected it therefore to be a star, its brightness and magnitude coinciding exactly with one of the interior satellites.

"On 31st January I observed this supposed star about 100″ distant from the edge of the ball of *Saturn* but on the opposite side; the satellites, *Enceladus*, *Tethys*, *Dione*, *Rhea*, were visible, and in the respective proper positions due to their daily revolutions. This star had been occulted by the planet during the inter-

val between the observations; but such occultation would not have been visible in England, as the planet would have set, before the immersion took place.

"The apparent magnitude of the star was 9 to 10.

"*Eastbourne, Feb.* 7, 1856."

Notes on an Occultation of a Star by Saturn.

By the Rev. W. R. Dawes.

(*Extracts of Letters to the Astronomer Royal.*)

"Last evening, on turning my equatorially-mounted telescope upon *Saturn*, I perceived a small fixed star on the preceding side of the planet, and almost precisely in the line of the major axis of the ring produced. Allowing for the brilliancy of *Saturn*, I estimated it be of the 9th magnitude. It was distant from the western extremity of the ring about 40″. The third satellite, *Tethys*, was very near its greatest elongation on the same side.

"As it appeared evident that an occultation must happen, I measured with the wire micrometer the difference of R.A. of the star and the western extremity of the ring; and from this, and *Saturn's* daily motion in R.A., I found that the G.M. time of occultation by the ring would be about $15^h\ 6^m$. Having also observed the difference of N.P.D. of the star and the most southerly point of the edge of the ring (considerably to the west of the southern extremity of its minor axis), I found from this and the daily motion in N.P.D., the difference of N.P.D. at the expected time of occultation. The threads of the micrometer having been set to include this difference of N.P.D., and placed equatorially, the southern thread was made to touch the southern point of the ring; and the other consequently showed, with sufficient exactness, the point on the edge of the ring where the occultation would take place, and also indicated the path of the star behind the planet. It thus became evident that the star would disappear a little to the south of the western extremity of the major axis; and that it would not make its appearance at all in the interval between the ring and the planet, but would just graze the southern edge of it; allowance being made for the northward motion of *Saturn*, amounting to about 0″·18 in an hour. Though the interest of the phenomenon was thus greatly diminished, I thought it worth while to observe the occultation with the view of ascertaining whether so small a star would continue to be distinctly visible up to the very edge of the ring; and also, with some hope, that if the planet should be sufficiently well seen when about 7^h west of the meridian, the star might appear between the bright rings.

"At $11^h\ 25^m$, G.M.T., *Tethys* was very nearly south of the star, and at a distance of about 5″. The star was estimated to be rather brighter than the fifth satellite, *Rhea*.

"At $14^h\ 50^m$, G.M.T., *Saturn* had approached very close to

the star; which, however, was still at times distinctly separated from the edge of the ring: but usually the edge was sadly tremulous. Power 162.

"At $15^h\ 1^m\pm$ I had the last distinct and certain view of the star during a few moments of steady vision. It was just in contact with the edge, and looked like a huge mountain projecting from it. Great tremulousness of the image precluded another view of the star. The last-mentioned time is a little uncertain; for I kept my eye for about two minutes at the telescope, in hopes of one more firm glimpse; which, however, did not occur.

"The state of the air soon afterwards became so bad, that the division between the rings was only occasionally visible; and it therefore seemed useless to await the passing of that division over the star. Had the planet been near the meridian, and as steady and well defined as it had been when in that position, I have little doubt that the star, though of so small a magnitude, might have been seen between the rings. It is to be hoped that this interesting phenomenon has been observed in America.

"When *Saturn* has removed to a sufficient distance from the star, I shall endeavour accurately to gauge its magnitude.

"The telescope employed in these observations has a focal length of nearly 10 feet, with a clear aperture of 8 inches, and is the work of Mr. Alvan Clark of Boston, U.S. Its defining power is very fine.

"Hoping that this observation, though far less complete than I could wish, may yet possess sufficient interest to excuse my troubling you with the present communication.

"I remain, yours faithfully, W. R. Dawes.

"*Wateringbury, Maidstone, 31 January, 1856.*"

"The early part of last night was very clear here, and I took some pains to obtain an accurate gauge of the magnitude of the fixed star, which was occulted on the 30th ult. To relieve the eye from the light of *Saturn*, a very small field of view was employed: but when the star was placed in its centre, the sky around it was evidently somewhat illuminated by the planetary twilight. The aperture on the telescope (the finder of my equatoreal), which rendered it just steadily visible, gave its magnitude $9\frac{1}{4}$ of my scale. The planetary illumination of the field, though at a distance of above 9′ from *Saturn's* centre, might make it appear a quarter of a magnitude smaller than the truth,—but scarcely, I think, half a magnitude. I intend, however, to resume this examination in a week or so, if opportunity should be afforded, when *Saturn* will have removed to more than twice his present distance from the star, and the moon will scarcely have become troublesome.

"I confess I was not prepared for the distinct visibility of so small a star when close to *Saturn's* ring; especially after Professor Secchi's remarks on the extreme faintness of the seventh-magnitude star, when it was very close to the planet on the 13th

Nov. 1854. (See *Astron. Nachr.* No. 982, p. 346, where it is not stated whether that star was actually *occulted* by *Saturn*.) When the star I observed was close to *Tethys*, I estimated it to be fully one magnitude, or perhaps one and a half larger than that satellite."

"*February* 4."

"I have carefully, and under favourable circumstances, gauged the magnitude of the small star, which was occulted by *Saturn* on January 30. I find it is rather brighter than the $8\frac{3}{4}$ mag. and decidedly below the $8\frac{1}{2}$. *Saturn* was excluded from the field during the observations; and the sky round the star was not perceptibly illuminated by the light from the planet: yet, to make sure of not underrating the magnitude of the star, it may, I think, be safely stated decimally as 8·6.

"At the same time, and by the same method of apertures, I gauged the magnitude of the companion of *Polaris;* which resulted, as nearly as possible, one magnitude lower, or about $9\frac{3}{4}$. According to the ratio I employ, the star occulted by *Saturn* is therefore fully four times as bright as the companion of *Polaris*. Its approximate place is

"R.A. $5^h\ 32^m\ 18^s$; N.P.D. 67° 48′ 24″.

"*March* 31."

Mr. De La Rue laid before the Meeting a coloured drawing of *Saturn*, as seen with his 13-inch Newtonian equatoreal, on March 28 and 29, 1856. Adopting the outer diameter of the outer ring as the unit for comparison, Mr. De La Rue finds that the relative dimensions which best agree with his observations are as follows:—

Outer diameter of outer ring	1·00000
Diameter of fine division	0·95417
Inner diameter of outer ring	0·88702
Outer — — middle —	0·87099
Inner — — — —	0·66106
— — — faint —	0·55586
Equatoreal diameter of the planet	0·44338

There was also exhibited at the Meeting a drawing of *Saturn* as seen at the Observatory of Mr. Barclay, Knotts Green, Leyton. The telescope employed was a refractor of $7\frac{1}{2}$ inches aperture, by Cooke of York, mounted equatoreally and driven by clockwork. The general resemblance which the drawing bore to other authenticated delineations of the planet was very satisfactory, and gave promise of future usefulness.

On the Outer Ring of Saturn. By Mr. John Watson, Washington Chemical Works, Durham.

(*Communicated by the Astronomer Royal.*)

"As the question of the division of the outer ring of *Saturn* cannot be looked upon as settled yet, it will not be necessary, I hope, to offer any apology for adding my mite of evidence to the mass already before the astronomical world. Some observers believe that the outer ring is divided in a manner similar to the well-known division between the outer and inner bright rings, but divided only by a comparatively narrow space; while others, possessed of powerful instruments, are of opinion that the outer ring, if marked at all, is only marked by a narrow *streak*, similar in appearance to the *streaks* or *belts* which so conspicuously cover the balls of *Jupiter* and *Saturn*. Some have even ventured so far as to divide the respective observers into the achromatic telescope and the reflecting telescope parties; the latter class not being able to see the division or mark, from some slight imperfection in the instrument. We ought not, however, to overlook the objections of Capt. Kater in 1825-6, with Newtonian telescopes of $6\frac{1}{4}$ and $6\frac{3}{4}$-in. aperture (*Astronomical Memoirs*, vol. iv.); nor the observation of Messrs. Lassell and Dawes, in Sept. 1843, with Mr. Lassell's 9-in. telescope. Both of these sets of observations, it will be seen, were made when the rings were pretty well opened as at present.

"The planet being in a good position during the last few months, I have paid close attention to the outer ring, with the view of judging, for myself at least, which of the opinions is most probably correct. My telescope is a 12-feet Newtonian reflector of 12-in. clear aperture, and possesses very excellent defining power. I believe very few large telescopes are able to show a star with a smaller spurious disc, as may be judged from the fact that η^2 *Andromedæ* has been neatly *split* with the full aperture, and a power of only 420, and well divided with powers from 500 to 900.

"I have obtained glimpses of the marks on the outer ring of *Saturn* on several occasions during the past winter, and my first impression was, that it was as really a division as that which separates the principal bright rings, but that it was closely bordering on the *minimum visible*, even with our best telescopes. I was led to suppose that the principal division would present much the same appearance with a telescope of barely sufficient magnifying and penetrating power to render it visible. To settle that point, I have carefully examined its appearance with a very fine $2\frac{3}{4}$-in. achromatic telescope by Cooke, using powers barely sufficient to give a steady view of the rings. For the same purpose, I have also contracted the aperture of my 12-in. telescope to only about 3 in. clear aperture (making allowance for the size of the small plane speculum) and reduced the magnifying power in the same proportion, until it was just possible to see the division by glimpses. When viewed

under these circumstances, I observed that the principal division presented the appearance of an exceedingly fine *black* line or mark, *without sensible breadth.*

"Under the most favourable circumstances, with the full aperture of the telescope, and powers varying from 300 to 900, I have never been able to detect on the outer ring that black appearance which I am led to suppose, even a very narrow division would present; and, on more than one occasion, I have obtained the impression *that the mark on the outer ring possessed sensible breadth.* I have also reason to suppose that *its appearance is variable.*

"At the conclusion of a very favourable season, my impression is, *that the outer ring of Saturn is not divided, but merely marked by a dark belt or streak of variable intensity.*

"*20th March*, 1856."

Observations of recently discovered small Planets, made at the Royal Observatory during the Month of March, 1856.

The North Polar Distances are corrected for Refraction and Parallax.

Urania.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	° ′ ″
1856, Mar. 7	9 5 19·0	8 8 21·52	70 20 3·08

Pomona.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, Mar. 14	9 56 28·4	9 27 15·15	83 22 18·01
31	8 45 42·2	9 23 18·82	82 4 11·01

Victoria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, Mar. 14	10 7 49·8	9 38 38·50	89 2 40·80
26	9 14 4·4	9 32 2·88	87 45 25·24
27	9 9 44·	9 31 39·16	87 39 49·18
29	9 1 11·4	9 30 57·44	87 28 11·33
31	8 52 44·5	9 30 22·20	87 16 51·48

Leda.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, Mar. 27	7 44 20·9	8 6 1·22	73 49 51·95

Egeria.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, Mar. 14	10 57 29·3	10 28 26·14	54 12 49·57
26	10 1 10·9	10 19 17·10	55 26 37·43
27	9 56 41·1	10 18 43·16	55 34 29·33
29	9 47 47·1	10 17 40·74	55 51 9·85
31	9 39 1·2	10 16 46·57	56 8 14·78
Apr. 1	9 34 38·9	10 16 20·10	56 18 26·38

Euphrosyne.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, Mar. 7	11 27 1·0	10 30 26·77	42 45 33·48
14	10 51 53·8	10 22 49·68	43 38 11·52
26	9 55 5·5	10 13 10·72	45 40 58·52
31	9 32 50·1	10 10 34·38	46 41 25·77
Apr. 1	9 28 27·9	10 10 8·07	46 53 59·58

Polyhymnia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, Mar. 7	9 18 56·0	8 22 0·78	68 21 49·08
29	7 48 40·2	8 18 14·26	68 24 44·65

Phocea.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
	h m s	h m s	° ′ ″
1856, Mar. 26	8 36 35·8	8 54 28·09	99 7 51·81
29	8 24 23·3	8 54 3·27	98 35 49·38

Thetis.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	° ′ ″
1856, Mar. 7 14 39 37·4	13 43 34·80	92 6 21·68
11 14 22 35·4	13 42 16·29	91 43 39·69
14 14 9 (40)	.....	91 25 22·90
27 13 11 9·3	13 33 43·27	89 57 36·66
31 12 52 24·8	13 30 41·89	89 29 25·42
Apr. 1 12 47 41·3	13 29 54·15	89 22 25·94

Lætitia.

Mean Solar Time of Observation.	Apparent R.A.	Apparent N.P.D.
h m s	h m s	° ′ ″
1856, Mar. 26 10 31 2·6	10 49 13·77	79 27 27·17
31 10 8 23·6	10 46 13·80	79 1 53·24

March 26. There is a little doubt whether this object is the planet.

G. B. Airy.

1856, *April* 14.

Measurements of Saturn with the Astronomer Royal's Double-image Micrometer, employed with a 13-in. Newtonian Equatoreal, March 28, 1856. By Warren De La Rue, Esq.

	Outer Diameter of Outer Ring.		Mean of 10 Measures Reduced to Mean Dist. Log = 0·979,6849	
	″		″	
Major axis	... 41·06	...	39·51	
Minor axis	... 18·71	...	18·00	Mean of 6 Measures

Distance of the boundary of the equatoreal bright zone from the northern edge of the outer ring:

Mean of 6 Measures ... 6″·94

corresponding to the 11° of south latitude.

Ueber Dr. Wichmann's Bestimmung der Parallaxe des Argelander'schens Sterns, von W. Döllen. (Bulletin Physico-Mathétique, tome xiii.) St. Pétersbourg, 1854.

This paper is devoted to a critical examination of Dr. Wichmann's researches on the parallax of Argelander's star (1830 *Groombridge*). The author states that it was placed in the hands of M. W. Struve as early as the month of June 1853, although, for a reason explained by him, its publication was delayed for some time.

After a few preliminary remarks, the author commences by giving a concise statement of the nature of the inquiry undertaken by Dr. Wichmann, and of the conclusions at which he arrived. The observer measures the distance between two stars, p and s, separate from each other by an interval of about 3200″. The results obtained by measures of this kind exhibit variations of a periodic character, and these being discussed upon the supposition of their being due to parallax, it has been found that the difference of parallax of the two stars amounts to 1″·17 with a probable error of 0″·08. Again, the observer measures the distance of the two stars p and s from a third star A (Argelander's star), situate nearly midway between them; and the periodic variations in the *differences* of these distances, which are very nearly equal in quantity and direction, lead to the conclusion that the excess of the parallax of A over half the sum of the parallaxes of p and s amounts to 0″·135, with a probable error of 0″·013.

The author, in his examination of the reasoning by which these results were deduced, has recourse, first, to considerations of a general nature, and then endeavours to substantiate his views by a process of calculation. He commences with an allusion to the many fruitless attempts which have been made to ascertain the parallax of the fixed stars. The memoir of Dr. Peters on this subject affords a highly instructive picture of the persevering efforts of the human mind in search of truth. These efforts, although unattended with success, in so far as the object of pursuit was concerned, have, notwithstanding, proved very advantageous to Astronomy, from the valuable improvements of the methods of investigation which they have suggested, and the astonishing perfection which they have occasioned in the fabrication of instruments of observation.

In Stellar Astronomy the progress of improvement has been such, that the objects of investigation may now be considered to be of the same order of magnitude as the disturbances by which the condition of the instrument in each individual instance of observation is inevitably affected. The peculiar feature of modern observation consists in the circumstance that the principal difficulties which offer themselves to the observer, reside not in the heavens, but in the instrument with which his observations are made. These must be overcome before any ulterior success in his inquiries can be expected.

To the astronomer the most perfect instrument is not that which is affected by the smallest errors, but that whose errors he is best acquainted with. It is not sufficient, however, that the observer has been enabled, *in any given instance*, to determine the errors of his instrument. In order to render his observations available for theoretical purposes it is desirable that he should know how to eliminate the errors by which his instrument is affected on each individual occasion of observation. This being generally impracticable, it only remains to make as little use as possible of the supposition of the invariability of the instrument. A clear perception of the necessity for adopting this course has led to the two most extensively-employed principles of modern observation, namely, the principle of the immediate reversal of the instrument, and the principle of the simultaneous measurement of the differences of two nearly equal quantities affected by similar circumstances. It must be acknowledged that the latter of these is in practice preferable to the former. In fact, the instrument *after* reversal, and even by virtue of that operation, is no longer in the same condition as that in which it previously was; and, notwithstanding that we have thereby eliminated certain sources of error, still it is to be feared that we have at the same time introduced new errors of an unknown character. It is true, indeed, that in the present state of observation an advantage is gained by the process of reversal; but it is possible that the day may come when this will no longer be so.

No such contingency can, however, occur in the case of the differential method of observation, the advantages of which become more obvious as the observations are more accurate, and as the quantities to be measured are smaller. And yet it is important to remark that, from the nature of this method, the conditions which assure its superiority are not rigorously fulfilled. In each particular case the quantities to be measured are not precisely equal; they are not affected by precisely the same circumstances, nor are they observed at precisely the same times. It is manifest, therefore, that there exists a determinate limit beyond which the advantages of the method cease to be appreciable. Hence arises the desirableness of obtaining some test by which we might be enabled in any instance to ascertain whether this limit has been attained. This object is frequently effected by a comparison of the probable errors of the same result, as determined by different combinations of the observations. Should any discordance manifest itself in the values of the probable errors as thus deduced, it is thereby demonstrated that the errors of the observations are not merely accidental, but, to a certain extent, regulated by a fixed law. Without knowing this law, it is so far instructive to know that by the combination which assigns the least probable error, the unknown errors of the observations are most effectually diminished. By this means we may generally rely upon having rendered the errors wholly or partially innocuous, although we are unable to ascertain either their nature or their magnitude.

The author contends at great length against the views of Dr.

Wichmann relative to the existence of a sensible parallax of the star *p*. He is of opinion that such a result is of itself extremely improbable, and he adduces various considerations in support of his assertion. He remarks, for instance, that if the star had the parallax which has been assigned to it by Dr. Wichmann, it ought to have a sensible proper motion. The motion of the solar system in space, which is no longer merely an hypothesis, being directed to a point in the heavens which is distant 77° from the star, almost the whole effect of such a motion ought to exhibit itself in an apparent displacement of the star, to an extent depending on the largeness of the star's parallax. But, since the star has been found to have no sensible apparent motion, it follows that it must have a real motion, the projection of which upon the celestial sphere is equal in magnitude, but opposite in direction, to the effect produced by the motion of the solar system in space. "Now such a coincidence," says the author, "is naturally not impossible, and we should certainly not be warranted in maintaining its improbability in opposition to a result deduced from observation, if we had no reason to doubt the truth of that result. But, on the other hand, to declare that an hypothesis which leads to such a conclusion, is one of a plausible character would be equally untenable."

In the second part of his memoir the author proceeds to consider the disturbing causes which he conceives may have affected the heliometric observations forming the groundwork of Dr. Wichmann's investigation. These he refers to the influences of temperature and flexure. With respect to the former, he is of opinion that the value of the thermometer coefficient as found by Bessel is entitled to reliance as a normal determination of the element; but that the case is quite different when the question refers to any specific measurement. The value of a revolution of the screw, which requires to be introduced into the calculations, is a function of the temperature, not merely of the screw, but also of the material constituting the object-glass; and these two arguments always operate in contrary directions. If the temperature rises, the screw extends, and the value of a revolution is thereby *increased;* on the other hand, an increase of temperature produces an extension of the focal length of the object-glass, and hence arises a *diminution* in the value of a revolution of the screw. According to Bessel the effect produced by those two opposing causes is in favour of the latter; in other words, an *increase* of temperature will tend on the whole to *diminish* the value of a revolution of the screw. The author remarks that the partial compensation which takes place under such circumstances had suggested to him certain ideas on the subject which he does not remember to have found elsewhere. Should these individual effects be nearly equal, and, what is quite possible, considerably greater in each case than their difference, then, in the application of the temperature correction, an error in the thermometric quantity employed as the groundwork of calculation is of much less

consequence than an error in the tacitly-assumed supposition that the screw and the object-glass have both the *same* temperature. Now this supposition is rarely, if ever, realised; and it is doubtless exceedingly difficult to ascertain the exact amount of difference in any individual case. The author describes an experiment illustrative of the different effects produced by exposing to the same temperature two thermometers, having bulbs of *unequal* magnitudes. He contends that a similar effect must be produced upon the screw and the object-glass of the heliometer, which are so dissimilar in form and magnitude, and the materials of which possess such unequal powers of conducting heat. He remarks further, that we ought to take into consideration not only the difference of temperature of the *different parts* of the instrument, but also the change of temperature which occurs *during the time of observation.* The adjustment of the eye-piece, with respect to the object-glass, which depends upon the temperature of the latter, is effected before the commencement of each set of observations. On the other hand, the temperature of the screw may be considered as coincident with the indications of the thermometer corresponding to the time of each individual observation. Supposing each set of observations to occupy an hour, the reading of the thermometer corresponding to the middle of this interval may be taken for determining the mean temperature of the screw. The question then arises, how high shall we estimate the difference between the temperature of the screw at a given instant, and the the temperature which determines the focal length of the object-glass half-an-hour earlier. Supposing this to amount to $\pm 10^{\circ}$ Fahrenheit, the author finds that the correction to the temperature coefficient hence arising may amount to $\pm 0''{\cdot}3$, a quantity which he shows to be of the same order as the principal part. He maintains, therefore, that he was justified in asserting that the question with respect to the influence of temperature upon the instrument cannot be considered as exhausted.

From the question of temperature, the author passes to a consideration of the influence of flexure. After a full discussion of this subject he takes up a series of the observations contained in Dr. Wichmann's paper, and forms, by means of them, a set of equations of condition which he solves upon various suppositions with respect to temperature, flexure, and parallax. The final result of these various essays may be best stated in his own words. "It has been demonstrated," says he, "that the observations under consideration cannot be satisfied by any possible value of the temperature co-efficient, nor by my formula for flexure, nor by parallax, nor by all together; other disturbing influences, regulated by a fixed law, must be in operation. The existence of such disturbing influences being once established, it is certainly not admissible to attach to any determination deduced from these observations a degree of confidence proportionate to the probable error obtained by means of them. In fact, upon what grounds could

we expect that these unknown disturbances had operated exclusively in the direction of proper motion, and not equally in the direction of parallax, or flexure, &c. For my own part, I am of opinion that to attempt to solve the enigma by the assumption of any other law of flexure, or by a modification of the correction for temperature, were an undertaking which, from the foregoing details, would seem to afford no prospect of success. I must stop here; I must content myself with having shown by this example what I had already established by considerations of a general nature, that in the present condition of our knowledge respecting the influence which an instrument such as the heliometer exercises upon the results of observation, it were utterly impossible to arrive at a reliable conclusion with respect to the cause of the periodic variations amounting to one second of space which the measures of a distance extending to about 59 revolutions of the screw, or 52′, exhibit in the course of a year. The undeniable presence of such periodic variations is, however, a fact of such weighty importance that we must not endanger our possession of it by a hasty decision. I here repeat what I have so often and so emphatically stated already, that only a renewed investigation of the subject, carefully prosecuted with reference to the special circumstances of each case under consideration, can lead to a definite result; but we may expect by this means to arrive at a conclusion of a trustworthy nature."

The author proceeds to remark that his criticisms must not be considered as implying that the heliometer is incapable of furnishing any essential contributions to our knowledge of the parallax of the fixed stars. On the contrary, he is of opinion that, in consequence of the large scope which it allows for selecting the stars of comparison, and the extraordinary accuracy with which its adjustment can be specially effected, it is peculiarly adapted for such inquiries. He then shows that, by combining together the results of Schlüter and Wichmann relative to the parallax of A, a value of the latter will be obtained, which is almost entirely free from the error of the instrument, *according to whatever law depending on the distance we may imagine it to operate.* In this way he finds the parallax of the star to be 0″·141, with a probable error of 0″·013, supposing the parallaxes of the stars of comparison to be evanescent,—an assumption which, in the present state of our knowledge, he considers to be perfectly justifiable. He remarks that the value of the parallax deduced from a suitable comparison of this result with the corresponding results of Peters and Otto Struve which he also considers to be worthy of confidence, must be considered as one of the most trustworthy determinations connected with the subject; and although it may not chime very accordantly with our notions in the present day respecting the distances and real proper motions of the stars, this circumstance merely indicates to us that we have still much to rectify in respect to those notions.

The author concludes his paper with a vindication of M. Otto

Struve's investigation of the parallax of Argelander's star, which had formed the subject of some critical remarks by Dr. Wichmann.

There is also an Appendix, in which he replies to some objections offered by Dr. Peters to the labours of the same astronomer in No. 865 of the *Astronomische Nachrichten.*

Notes on the Meteor of January 7, 1856.

By Robert James Mann, M.D.

A very brilliant meteor crossed the sky of the south of England during the twilight hour of January 7th, 1856. The writer, happening to be amongst those who were so fortunate as to see it, has thought it well to transmit a few notes on the subject to the Royal Astronomical Society, upon the chance of there being some point that came within the scope of his observation and inquiries which may prove worthy of the notice of such of the fellows of the Society, as particularly interest themselves with meteoric phenomena.

The meteor presented itself at Ventnor, on the south coast of the Isle of Wight, at 5 minutes before 5, local time. The sky was perfectly clear at the instant, excepting for a low cloud-bank, extending from the west pretty well round the horizon. The sun had set nearly three quarters of an hour before; but the twilight was still so strong, that the diminution of daylight was not very obvious. The planet *Jupiter* had passed the meridian an hour and forty minutes, and was only to be discerned after looking for it very closely in the exact position that it occupied. No other star was visible to the unaided eye.

When the meteor appeared, the writer was walking eastwards, along a road open to the sea towards the south. He was suddenly startled by the sense of a great blaze moving along high up in the sky to the right, accompanied with a slight rushing sound.

The flame assumed the form of a disc or ball, very distinctly. It was of an intense whitish-blue hue, and became brighter and bluer as it descended towards the horizon. The ball, too, seemed to assume progressively larger dimensions. When, at its largest development, it appeared to have a diameter of from seven to eight minutes of angular measure. The glare that it cast was as strong as that of a very vivid flash of lightning; but was, of course, more sustained. The blazing ball moved along with about the ordinary speed of the falling stars, when, about seven degrees above the horizon, and a little to the east of the meridian, it plunged into the low cloud-bank already alluded to, having, however lost some of its brightness before doing so, and finally disappeared.

When the meteoric flame had fallen to *within 35 degrees of the horizon*, it began to leave a tail of fire behind it, very similar to the trace that is left by a Roman candle on a dark night. The

upper and lower extremity of this tail faded out of sight almost as soon as it had been observed; but the central portion, through a range of from fifteen to twenty degrees, remained like a clear line of incandescent silver burnt into the sky. At first the line was almost "mathematical" in its fineness, having no discernible breadth, and was obviously composed of intensely incandescent substance; although so fine, it was remarkably bold and distinct to observation, in consequence of its brilliant whiteness. In a few seconds, however, it no less manifestly had acquired width, and then it went on growing broader, until almost in a couple of minutes it presented itself as a silvery white spindle-shaped cloud, standing boldly out from the clear sky. On this part of the phenomenon the writer's attention was very particularly fixed, and he is convinced that he saw the luminous trace pass gradually from the condition of fire, into that of reflective vapour.

When its nebulous character was fully declared, it had at first the character of a long, slender wand, very nearly straight, and stretching through an extent of between fifteen and twenty degrees. The central portion of this wand, however, soon began to bulge out, until the whole assumed the form of a spindle, tapering at each extremity. As it did this, the cloud-composition was more and more clearly declared, the cloud-substance being gathered together into subordinate masses, with clear streaks between them, until the general aspect of the object was of a freckled and then mottled character. As this change occurred, the extremities of the spindle began to bend themselves opposite ways, and the band itself become sinuous. After the lapse of two or three more minutes, the sinuous folds were developed, and the ribbon of cloud had obtained its full breadth. The general outline now very nearly approached to that of a serpent gathering itself up, and rearing upon its tail. Soon after this the central fold became more curved than the rest, and looked like the bend of a horse-shoe. At this period the sinuous object was evidently drifting away towards the south-east, and rising, balloon-like, to a higher elevation as it went, the middle portion having a more rapid motion than the extremities. The sinuous foldings were clearly effects of the different movements of the atmospheric strata through which the meteor-track was stretched. From this time the apparent dimensions of the cloudy object became smaller and smaller in consequence of increasing distance, and also its light waned progressively, and its extremities were shortened by advancing solution. Sixteen minutes after the fall of the meteor there remained nothing else visible in the sky but a very faint streak of inconspicuous, airless cloud, and in two minutes more was nothing but vacant air. The beautiful apparition had vanished entirely.

From a comparison of the observed position of the meteor in various places, the writer concludes that it fell to the earth within about ten or twelve miles of the coast of Normandy, and not very far from Isigny.

Bemerkungen über die Parallaxe des Argelander'schens Sterns, und über die Heliometer Beobactungen, von Herrn Dr. Wichmann.

In an earlier part of this *Notice** is an abstract of an elaborate memoir by M. Döllen in opposition to the views of Dr. Wichmann respecting the parallax of the star *Groombridge*, and it is thought desirable to add an abstract of a recent paper by Dr. Wichmann on the same subject, which is printed in the *Astronomische Nachrichten*, Nos. 1010 and 1011. It will be remembered that three years ago Dr. Wichmann published a remarkable paper in the *Nachrichten* on the parallax of this star, in which he discussed the observations made by himself and M. Schlüter with the heliometer. The result was curious and startling, inasmuch as it seemed to show that the more westerly of the stars of comparison had, relatively to the principal star, a parallax amounting to about a second of space, or, that this star, though of very low magnitude, was very much nearer to the solar system than the principal star of which the parallax was sought. Every pains had been taken by Dr. Wichmann to eliminate all sources of error which might have caused such an anomalous result, yet after all his pains he could only arrive at the conclusion that the supposition of the large parallax of the smaller stars led to fewer difficulties than any other hypothesis which could be made. Astronomers were naturally led to view jealously and closely this remarkable result, and, in addition to the notice taken of it in the annual report of this Society for February 1853, two profound critiques were written by Dr. Peters and M. Döllen, the former of which is printed in the *Astronomische Nachrichten*, No. 865 ; and the latter (abstracted in the present *Notice*), in the *Bulletin Physico-Mathém. de l'Académie de St. Pétersbourg*, tom. xiii.

Dr. Wichmann seems to have hoped to be able to substantiate or disprove the result referred to by a series of observations of his own, but being disappointed in this, he at length feels it necessary to state the views which he entertains of the question at the present time.

He remarks that Dr. Peters had proved the uncertainty of the effect of temperature on the screw of the heliometer, as deduced by M. Bessel, and had shown that in the observations of the *Pleiades* constant differences were found in groups of observations made at different times. M. Döllen had also endeavoured by general considerations to show the uncertainty and want of probability of the parallax found, and to give another explanation of the unmistakeable periodical changes exhibited in the measures; yet still the *possibility* remained, that the star of comparison might really have a conspicuous parallax, and on this account a *direct* contradiction to the questionable result by means of new observations was very desirable. This has been at length effected by the numerous and excellent measures made by Mr. Johnson with the Oxford helio-

* See 156 *et seq.*

meter in the years 1852 and 1853, published in the fourteenth volume of the *Observations of the Radcliffe Observatory*. While, from the observations made with the Königsberg heliometer, it resulted that the parallax of the western star of comparison was greater by a second than that of the eastern, Mr. Johnson finds, on the contrary, a small excess of parallax for the eastern star. These two contradictory results show satisfactorily that no confidence can be given to the conclusions respecting the parallax of the comparison-stars derived from the sums of the observed distances. A further scrutiny of the Oxford observations has shown Dr. Wichmann that the sums of so great distances can be found by means of the Oxford heliometer with greater accuracy than with the Königsberg instrument, and that consequently the observations made with it are fully entitled to give a decision concerning the questionable difference of parallax of the stars of comparison. "*I have by this means thoroughly convinced myself*," says Dr. Wichmann, "*that my hypothesis of the remarkable excess of parallax of the western star is no longer tenable; and further, that the periodical changes of distances exhibited so unmistakeably in the observations have their origin in instrumental causes.* I think it necessary to make this acknowledgment, and I do it so much the more willingly, because my former work might easily give occasion to, and has, in fact, occasioned an erroneous conception of my views. I consequently here repeat expressly that I first took refuge in the hypothesis of the parallax of the star of comparison because all my pains, as well during the observations as the reduction of them, to discover the source of the periodical changes, were fruitless, and I therefore at last held the existence of the parallax of the comparison-star to be at least quite as probable as, nay even more probable than, the existence of periodical errors in the measures, which must certainly appear far less perplexing to one who is not accurately acquainted with the instrument, than to him who, through constant use of it, has learnt to estimate its excellence. The doubts which I entertained concerning the accuracy of my result I could only justify through a feeling of mistrust, and therefore repressed them altogether; whilst, on the contrary, I brought forward everything that seemed to speak in favour of its probability, in the hope that by this means a more extensive examination of it would be the sooner elicited. I do not believe that by this means the independent judgment of an attentive reader would be prejudiced, since the details of my observations and calculations have been given with sufficient fulness to enable him to separate what is certain from the more doubtful matter, and I leave it, therefore, to the judgment of any reader to determine whether the reproaches made to me by M. Döllen, which besides do not affect the work itself, but the conclusions derived from the computations, are well grounded or not. The tone, little suitable to a scientific object, in which those remarks have been expressed, will in the meanwhile be my excuse for not inserting in this place those remarks, which are utterly irrelevant to the subject.

"If we thus give up the parallax of the comparison-star derived from the distances themselves, there remains as the result of the Königsberg heliometer observations, only that arising from the differences of the measures, namely, that the parallax of *Groombridge* 1830 is greater by $0''{\cdot}14 \pm 0''{\cdot}013$ than the mean of the parallaxes of the comparison-stars. This result is not excelled in accuracy by that found by Mr. Johnson, $\pi = 0''{\cdot}033 \pm 0''{\cdot}028$, as even the probable error shows, since the Königsberg instrument, with reference to small distances, or in the estimation of the differences of large distances, (at least with the method of observation employed up to this time,) seems to be decidedly superior to the Oxford heliometer. I should not, however, be surprised if, after all, many astronomers will doubt whether the science of observation is sufficiently advanced to *prove* the existence of a parallax of $0''{\cdot}14$, or whether the influence of one or more unknown causes may not by accident be so nearly coincident with the march of the parallax as to make the calculations indicate a sufficiently great degree of probability."

The candid statement of Dr. Wichmann, which has been given above in his own words, sets at rest the question relative to the parallax of the western star of comparison, which had so greatly excited the interest of astronomers, and at the same time proves that even in the case of so admirable an instrument as the Königsberg heliometer, used with consummate skill, periodical errors may occur in the measurement of large arcs, which may be confounded with parallactic motion of the star observed. Such an investigation as that elicited by Dr. Wichmann's anomalous result is perhaps at this epoch of more value than would have been the successful determination of the parallax of Argelander's star (*Groombridge* 1830); the limits and the sources of all possible instrumental errors will be both for the Königsberg and the Oxford heliometer discovered with greater ease; and the way is opened for more successful attempts at the discovery of very small parallactic motions.

New Planet.

On the 31st of March a new planet was discovered by M. Goldschmidt. The following approximate position of it was obtained by him on the same evening: —

	Paris M.T.	R.A.	Decl.
	h m	h m s	° ′
1856, March 31	10 5	13 13 30	−0 2

The planet discovered by M. Chacornac on the 8th of February has been called *Lætitia*.

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London:—Printed by G. Barclay, Castle St. Leicester Sq. and Published at the Apartments of the Society, May 8, 1856.